### The Principle of Activity and Lagrange's Equations. Rotation of a Rigid Body.

THERE are some people who understand by Newton's second law of motion nothing more than the three equations of motion for a body which can be treated as a particle, viz., $m\ddot{x} = \mathrm{X}$, &c. (or rather the equivalent equations for impulsive forces). Such people, however, would probably not seriously object to any dynamical truth whatsoever, from the conservation of energy to the principle of varying action, being read into this law, if only he who does so would explain clearly his own interpretation of Newton's statement. I, for one, am a little curious to have stated fully the principle which justifies Mr. Heaviside in his letter in your issue of January 29 in deducing from the solitary equation

$$\dot{\mathrm{T}} = \left(\frac{d}{dt} \cdot \frac{d\mathrm{T}}{dv_1} - \frac{d\mathrm{T}}{dx_1}\right)v_1 + \ . \ . \ .$$

that "*by Newton*, the force on $x_1$ is the coefficient of $v_1$."

It is a sufficient indication either of an incorrect premiss or of bad logic, however obscure an argument may be, if the conclusion be wrong; one does not readily see from Mr. Heaviside's letter how he could object to his method being applied directly to the motion of a rigid body with one point fixed, in which case, as is well known, taking

$$2\mathrm{T} = \mathrm{A}\omega_1^2 + \mathrm{B}\omega_2^2 + \mathrm{C}\omega_3^2$$

it leads to a wrong expression for the external couple round the axis of $x$, viz. $\mathrm{A}\dot{\omega}_1$ instead of the correct one, $\mathrm{A}\dot{\omega}_1 - (\mathrm{B} - \mathrm{C})\omega_2\omega_3$.

W. McF. ORR.

Royal College of Science, Dublin, February 2.

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PROF. ORR's opening remarks perhaps indicate that the want of appreciation of Newton's dynamics is even greater than I supposed. My authority for Newton is that stiff but thoroughgoing work, Thomson and Tait. On comparison, I find that Prof. Orr's "some people" seem to overlook the vitally important third law, without which there could be no dynamics resembling the reality, and also the remarkable associated scholium "Si æstimatur . . .," enunciating the principle of activity, which is of such universal and convenient application, both by practicians and by some theorists. In my short outline of the beginning of the theory of Lagrange's equations, my argument "by Newton" referred to the activity principle.

The example of failure given by Prof. Orr is remarkable in more than one way. If the three coordinates specified the configuration, then the equations of motion would come out in the way indicated. It is clear, therefore, from the failure that in the concrete example of a rotating rigid body, the coordinates employed, which are the time-integrals of the angular velocities about three moving axes, are not proper Lagrangian coordinates within the meaning of the Act. If we use coordinates which do fix the configuration (Thomson and Tait, § 319), there is no failure.

But it is quite easy to avoid the usual complicated trigonometrical work, and obtain the proper equations of motion by allowing for the motion of the axes. Thus, if **a** is the angular velocity, the angular momentum is

$$\mathbf{i}\frac{d\mathrm{T}}{da_1} + \ . \ . \ . \ = \mathrm{A}a_1\mathbf{i} + \mathrm{B}a_2\mathbf{j} + \mathrm{C}a_3\mathbf{k},$$

and the torque is its time differentiant, that is,

$$\mathbf{F} = \mathrm{A}\dot{a}_1\mathbf{i} + \mathrm{B}\dot{a}_2\mathbf{j} + \mathrm{C}\dot{a}_3\mathbf{k} + \mathrm{A}a_1\frac{d\mathbf{i}}{dt} + \mathrm{B}a_2\frac{d\mathbf{j}}{dt} + \mathrm{C}a_3\frac{d\mathbf{k}}{dt}.$$

Here **i, j, k** are unit vectors specifying the directions of the principal axes. They only vary by the rotation, so $d\mathbf{i}/dt = \mathrm{V}\mathbf{ai}$, &c., and this makes

$$\mathbf{F} = \mathrm{A}a_1(\mathbf{j}a_3 - \mathbf{k}a_2) + \mathrm{B}a_2(\mathbf{k}a_1 - \mathbf{i}a_3) + \mathrm{C}a_3(\mathbf{i}a_2 - \mathbf{j}a_3) + \mathrm{A}\dot{a}_1\mathbf{i} + \ . \ . \ .$$
$$= \mathbf{i}\{\mathrm{A}\dot{a}_1 - a_2a_3(\mathrm{B} - \mathrm{C})\} + \mathbf{j}\{. \ . \ .\} + \mathbf{k}\{. \ . \ .\}.$$

This exhibits Euler's three well-known equations of motion round the three principal moving axes.

In general, $\mathrm{T} = \frac{1}{2}\mathbf{a}\mathrm{M}\mathbf{a}$, where M is a vectorial matrix (or linear vector operator), fixed in the body. Then the momentum is $\mathrm{M}\mathbf{a}$, and the torque is

$$\mathbf{F} = \mathrm{M}\dot{\mathbf{a}} + \dot{\mathrm{M}}\mathbf{a} = \mathrm{M}\dot{\mathbf{a}} + (\mathrm{V}\mathbf{a}\mathrm{M})\mathbf{a}.$$

This allows M to be specified with respect to any axes fixed in the rotating body. Of course, the principal axes are the best. I may refer to my "Elec. Pa.," vol. ii., p. 547, footnote, for details of a similar calculation relating to the torque (and activity thereof) produced in an eolotropic dielectric under electric stress.

The following concisely exhibits the necessity of allowing for variation of M, and how it is done in the general case of $n$ independent variables:—Let $\mathrm{T} = \frac{1}{2}\mathbf{v}\mathrm{M}\mathbf{v} = \frac{1}{2}\mathbf{pv}$. Then **v** is a "vector" or complex of $n$ velocities, and $\mathbf{p} = \mathrm{M}\mathbf{v}$ is the corresponding momentum, whilst M is a symmetrical matrix. By differentiation to $t$,

$$\dot{\mathrm{T}} = \mathbf{v}(\mathrm{M}\dot{\mathbf{v}} + \tfrac{1}{2}\dot{\mathrm{M}}\mathbf{v}) = \mathbf{Fv} \quad \text{(Hamilton)},$$

or

$$\mathrm{T} = \mathbf{v}(\dot{\mathbf{p}} - \tfrac{1}{2}\dot{\mathrm{M}}\mathbf{v}) = \mathbf{Fv} \quad \text{(Lagrange)}.$$

Here **F** is the force on the system, in the same sense as **v** is the velocity of the system. For $\dot{\mathrm{M}}$ substitute $v(d\mathrm{M}/dx)$, to come to the usual forms by breaking up into $n$ components. But the above are more general, because M may vary independently of $x$. Activity should be the leading idea.

OLIVER HEAVISIDE.

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### Insects and Petal-less Flowers.

I WAS much interested by Mr. Bulman's account of Prof. Plateau's experiments in the matter of insects' visits to petal-less flowers in the issue of NATURE for February 5 (p. 319), wherein it is stated that Prof. Plateau contends that insects "are not attracted by the brilliant colours of the blossoms, but rather by the perception in some other way—probably by scent—that there is honey or pollen."

It has not been my good fortune to read Prof. Plateau's own account of the experiments which led him to the above conclusion, but it certainly appears to me, from your correspondent's summary, that he is generalising from an observation which has only a strictly limited application.

We are told that in the case of thirty poppies artificially deprived of their petals, as compared with seventy intact poppies, the average visits were as 4·5 is to 2·4, the most striking case instanced being that of the Dipterous insect *Melanostoma mellina*, the visits of which were as 4 is to 0.

The experiment and its result does not, to my mind, in the least tend to bear out the theory it is advanced to support, though your correspondent gives the method his approval.

I do not wish to doubt the possibility of smell playing a part in attracting insects, but I certainly cannot see that the artificial removal of the coloured petals proves that colour has no influence. We are fond of attributing great intelligence and power of perception to the bee, and yet in this case the insect is not even given credit for being able to recognise what are known to it, from possibly long experience, as the essential parts of the flower! Because we buy well advertised goods, and still continue to buy them when their proved virtue renders advertisement a thing of the past, is it proof that the advertisement played no part in determining our choice? The answer is obvious.

The greater number of insects visiting the poppies shorn of their petals might easily be accounted for, especially in the case of the Diptera, by the presence of some attractive substance in the sap exuded from the cut tissues, and probably by the resulting greater accessibility.

As a contrast to this experiment I would mention that of Lord Avebury, which loses none of its significance through being described in a popular magazine (the *London*, Christmas number). Quantities of honey were taken and laid on glass slips, and a marked bee was trained to come to a certain spot for it. The honey was supplied on slips of six different colours—blue, red, yellow, orange, green and white—and on one plain slip. Lord Avebury so arranged matters that the bee was persuaded to visit each and every slip before returning to the hive, the method being as follows:—

Seven slips in a row on lawn; the bee arrives and alights on (say) blue; it is allowed to remain for a few seconds and then driven off, the blue slip being withdrawn; it then goes to (say) white; after a few seconds at white the bee is again driven off, and goes to (say) yellow, the white slip being also withdrawn; after having visited all the slips in this way, and being at last deprived of every one, the bee goes back to the hive.

During the bee's absence the glasses are replaced, but in different order, and on the insect's return it is again noted which slip receives first attention.

Out of a hundred such complete rounds Lord Avebury

found his bee went to the blue glass *first* thirty-one times, and *last* only four times, while the plain glass came in for first notice only five times, and last twenty-four times. The other colours occupied intermediate positions in the bee's favour.

Here we have a case of which the bee could not possibly have had previous experience, and where every precaution was taken to avoid any undue advantage of position, &c., being given to any particular colour, with a result going far to prove that all other conditions being alike, colour does play an important part in deciding an insect's choice.

I would suggest that the correct method of settling the question would be to cut away, not the petals, but the stamens, &c. Then if insects continued to visit flowers so mutilated we should have grounds for thinking that petals exercise some attraction, or *vice versâ*.

E. ERNEST LOWE.

Municipal Museum and Art Gallery, Plymouth, February 9.

### Science and the Education Act of 1902.

IN two letters to you last year, I drew the attention of scientific men and of others interested in the welfare of our country and empire to the inferior position which scientific studies continue to hold in the education of the youth of this country (see NATURE, vol. lxvi. pp. 350, 459). One hoped that the Education Act of 1902 would do something to remedy present defects. That hope, it is to be feared, is in a poor way of being realised, so far as any inference can be drawn from the composition of the "Education Committee" recently appointed by the Council of a county so near to the metropolis as Hertfordshire. The whole thing is little better than a jumble, the sort of thing one would expect from the manipulation of a county-directory in a solicitor's office. So little did the County Council appear from the newspaper report to realise the gravity of the task before them that they adopted *en bloc* and without criticism the list prepared for them by the Clerk of the Peace, whose first-hand knowledge of education can only be at the best extremely limited. The committee-list bristles with names of county respectability, including a noble earl, a few M.P.'s, a fair sprinkling of J.P.'s, and among the C.C.'s elected very few appear to have taken a degree at any university, while one solitary name appears as a representative of science in that of Sir John Evans, F.R.S., who might have been a little more vigilant in this matter.

Outside the Council, we find the name of the Dean of St. Albans, a scholarly, clear-sighted, large-minded man, an acquisition to any committee; then the names of the two *classical* head-masters of Haileybury and Berkhampstead, men of the type referred to in my previous letters (*supra*), who cannot be expected to appreciate the importance of scientific education, but whose position in the educational world will give adventitious value to their opinions among the rank and file of the educational ignoramuses. In a list of some twenty-one, one solitary name, that of the young head-master of a not very important school in this neighbourhood, appears as a representative of science. It does not appear that a single representative of the Army or Navy or a single graduate in science or medicine finds a place on the committee; and such men resident in the county as my neighbour the principal of the Diocesan Training College (who is zealously engaged in attempting to train elementary teachers on scientific lines), or the official secretary of University College, or myself (with a record of more than a quarter of a century of public-school and scientific work) seem to have been the last people to be thought of.

In the light of the above facts, can it be unfair to say that the cause of progressive education in the county of Herts has drifted? And if this can happen in a county so near London, what is likely to happen in the more remote counties, where provincial ideas prevail more strongly? Is it not time that the leading scientific societies, led by the Royal Society or by the British Association, should draw up a memorandum impressing upon the county and borough councils of the country the serious call made to them by the Education Act to do their best to strengthen the sinews of the intellectual war, which (*nolens volens*) this country must be prepared to carry on? Had there been a single man of light and leading in the Cabinet, such instructions might have been included in the Act or its preamble as to render such action unnecessary. But so beclouded were the minds of our legislators in the long, dreary strife of bigotry and partisanship of last autumn that they seem to have lost sight of higher intellectual issues altogether. Let us hope that in the great provincial centres such an important point as the due representation of scientific education on the educational committees will not be lost sight of. A. IRVING.

Hockerill, Bishop's Stortford, February 6.

## RADIO-ACTIVITY OF ORDINARY MATERIALS.

IT is now well recognised that the air in any ordinary vessel possesses the power of conducting electricity, although to a very slight extent. It has been usual to refer to the effect as the "spontaneous ionisation" of the air. This name suggests that the conductivity is in some way an essential property of the air, just as the electrical conductivity of metals is inseparably connected with the nature of those bodies. Mr. C. T. R. Wilson, however, has found (*Proc.* Roy. Soc., vol. lxix. p. 277) that, when other gases are substituted for air, the relative ionisations are in nearly the same ratio as those which I observed for the same gases under the action of Becquerel radiation (*Phil. Trans.*, 1901, p. 507). Further, Mr. J. Patterson (*Proc.* Camb. Phil. Soc., vol. xii. p. 44) has found that, when a large vessel is used, the amount of ionisation is not proportional to the pressure, but tends towards a limit, when further increase of pressure no longer affects it. This is exactly the behaviour that might be expected if the effect was due to a feeble radio-activity of the walls of the vessel, the radiation being easily absorbed by the air.

I have recently carried out a series of experiments with a view to decide whether the nature of the walls of the vessel had any influence on the rate of discharge of a charged body inside it.

The various materials were made into cylinders, 13 cm. long and 3·4 cm. in diameter. A central wire, charged, and connected with an electroscope, formed the leaking system. The electroscope was exhausted, so as to avoid any leakage through the air in it, and, before each experiment, the insulation, which was of lead-glass tube, dried by the exhaustion of the vessel in presence of phosphoric anhydride, was tested. No leakage could be detected. On admitting dried air, a small leakage immediately set in, and its amount could be measured by timing the movement of the gold leaf over the scale division of a microscope with micrometer eye-piece focussed upon the leaf.

The leakage in scale divisions per hour, with various materials surrounding the charged wire, is given below:—

| | |
|---|---|
| Tin foil | 3·3 |
| Ditto, another sample | 2·3 |
| Glass coated with phosphoric acid | 1·3 |
| Silver, chemically deposited on glass | 1·6 |
| Zinc | 1·2 |
| Lead | 2·2 |
| Copper (clean) | 2·3 |
| Ditto, thoroughly oxidised | 1·7 |
| Platinum (various samples) | 2·0, 2·9, 3·9 |
| Aluminium | 1·4 |

It appears, then, that there are very marked differences in the rate of the leak, when different materials constitute the walls of the vessel. There can therefore be little doubt that the greater part—if not the whole—of the observed ionisation of air is not spontaneous at all, but due to Becquerel rays from the vessel.

It is, I think, interesting to find that the phenomena of radio-activity, which have generally been regarded as rare and exceptional, are really everywhere present.

The rate of leak with various pieces of tin foil from the same stock was always the same, as nearly as the experiments could show—that is, to within about 6 per